February 26, 1955

Dr. Norton Zinder
Rockefeller Institute
New York 21, N.Y.

Dear Norton:

As you probably know, Alan Garen has been inquiring about the possibility of spending a postdoctoral fellowship here next year. I would like to ask your advice about it. Have you discussed this with him yourself? Would he have learned from you or from Dave Skaar very well the kind of program we have had going here? I have tried to find from Garen himself just what he's after, but am afraid I did not get that question across. Actually, in addition to our own lab. facilities, it would not be too difficult to arrange for some work at the Enzyme Institute or in the new Bacteriology building, for biophysical experiments. The main thing I'd like to ask you about is just what you think of Garen, and what he might get out of a fellowship here (and what he would bring to us in turn).

I wonder if you know a girl at CSH who works for Evelyn -- Constance Thomas, who is applying for a graduate assistantship. If so, how do you think she would fit in?

Larry told me something of your experiment on "restoration of phage", but I am afraid I did not get a very coherent account of it. My brother Seymour has told me of some experiments they've done at Illinois on the restoration of UV'd bacteria with phage, and I wondered if this has anything to do with it. Larry also forwarded your bill of materials, and we are getting these together for you. However, I don't quite see how "stable transductions" fit in. In the first place, all the Gal-- 's give heterogenotic clones after transduction; in addition, apparently stable Gal+ have been found, in greater or lesser proportion with different stocks in different experiments. However, I am inclined to think that the stable occurrences are segregates out of initial heterogenotes which happened to fall apart early, during initial purification. Of course all the heterogenotes sooner or later throw stable segregants of the various parental and crossover types, so the distinctions, if any, are only a matter of timing. But Larry didn't tell me enough of your idea to see why the heterogenotes would not be quite as informative as the stable segregants. As he must have told you, we have not been able to find <u>any</u> unambiguous evidence of transduction by lytically grown lambda (though some observations of Esther's are still unsettled); since induced lambda involves treatment with UV, he has planned to check whether lambda grown on UV'd bacteria would be competent; this sounds like quite a different experiment from the one you're planning. If I understand this at all, you should pick up transduction by phage harvested from UV'd inputs. Since control lytic lambda does not show any obvious level of transducing activity, you should be able to get a fair answer from a fairly simple straightforward experiment using any Gal+ --x Gal-. (My hunch is that this experiment would be

more likely to work in a system where lytic lambda already shows some transducing activity. Have you tried it with Salmonella or with P1/E. coli?

With best regards;

Yours sincerely,

Joshua Lederberg

P.S. Don't you already have lambda, lambda-2, K-12, W-1485 stocks, or can't you get them readily from CSH ?